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ON COLD AS A CAUSE OF DISEASE.

PRESIDENT'S ADDRESS

TO THE

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BY

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GENTLEMEN,—I thank you all very much for the honour you have conferred upon me in electing me your president: for this I feel that I am much more indebted to your kind remembrance of my seniority than to any claims upon your consideration for services rendered to the Society or otherwise. It will be my pleasing duty to endeavour to justify your choice, by making every effort to perform the duties of the office so as to meet with your approval. I have reason to hope, that helped by Dr. Hatherley, the late President, and by our active and able Secretaries, the coming Session's work may be so conducted as to compare fairly with the last. One presidential duty, however, did give me some occasion for hesitation; that was the question of the usual address. However, I at length decided that it would be more likely to interest you if I departed somewhat from the routine by reading a paper upon a debatable

subject, and inviting a discussion thereon. The subject selected is one of wide and general interest, and from its own nature, as well as from the mode in which it is dealt with, suitable for treatment by one who enjoys the questionable advantages of age to excuse, if not to justify, the audacity of the attempt. I have entitled it an address

ON COLD CONSIDERED AS A CAUSE OF DISEASE.

§ 1. THE DOCTRINE STATED.—The doctrine on this subject usually taught in the Medical Schools, and generally accepted by the Profession, is not easy to summarize with precision; partly on account of its inherent vagueness, and partly on account of the varied forms given to it by different writers. However, it may be approximately stated thus. Cold is considered to be the most common single cause of grave disease in this country and in these latitudes. It is said to act both as a predisposing and as an exciting cause. The disease processes to which it is said to give rise are fever and inflammation. The parts which it is said to affect are, the respiratory, the alimentary, the renal, the generative, the locomotor, and the nervous organs; as well as the skin, and sometimes, secondarily, at least, the organs of circulation. The named diseases imputed to it are bronchitis, coryza, laryngitis, sore-throat, pleurisy, pneumonia, pul-

monary plithisis, rheumatisms, Bright's disease, gastric catarrh, diarrhoea, peritonitis, paralysis from central or peripheral lesion, menstrual irregularity, febricula of many forms, cardiac diseases, even gout; and, with more justification, frost-bite, chilblains, and chapped hands. Yet I have not mentioned by any means all the diseases attributed to cold, only those more generally so considered.

It is held that the cold may be applied either at, near, or distant from the part which becomes affected; that the locality of the action of the cold does not determine the seat of the subsequent disease, which is dependent upon the sensitiveness of the part or organ which suffers; that the extent of surface cooled may be but a few square inches or the whole body; that the degree and duration of its action are not important, and do not measure the severity of the resulting disease; so that from trifling exposures severe effects may follow; and that it may be applied in various ways, by cold air in motion, dry or damp, or by damp or wet contacts. The way in which inflammations of distant viscera are caused by the application of cold is not explained in any satisfactory manner; but the hypothesis of a *locus minoris resistentiæ* is adopted to explain the locality of the effect. I cannot pursue here the various speculative views upon this point, and it is less needful, as I willingly admit, that if the facts

be proven, the theory framed in explanation of them would be of less comparative importance. I do, however, hold that until it be a proven fact that cold does these things, no explanation is needed. In conformity with these doctrines the laity have been taught to be very careful to avoid this grave cause of disease, and, to my mind, it seems that they have become a little too fearful of fresh air, and prefer the risks of foul air, if only it be warm.

The popular theory is more definite, and held more firmly than the professional. It affirms that all the diseases above named, and many others known as chill diseases, are caused by cold, and can be prevented by keeping warm, or cured by the application of heat. A very natural and logical view. It admits that the cause is often applied, but the effect does not follow, that the effect often occurs when no special incidence of cold has been observed; but here, somewhat less logically, faith remains unshaken, and the conclusion is arrived at that "a cold must have been caught somehow." This is the state of mind which explains the common expressions "a feverish cold," a "cold in the head," in which the malady is indicated by the name of its assumed cause. Yet are such expressions so generally used that it is difficult to find an equivalent for them, and thus are illustrated the wide acceptance and long continued domination

of these views among the masses who make language.

§ 2. ITS FOUNDATIONS STATED. --- Such a doctrine as this should have solid foundations; but so far, I have failed to find such satisfactory evidence for it as might have been expected from the wide acceptance it has received, the importance of the question to life and health, and the confident tone in which it is enunciated. The older, even more than recent systematic writers, have imputed to cold nearly all the internal inflammations for which no other cause could be easily assigned, more especially the catarrhal and rheumatic forms; and this opinion appears to have rested largely upon the prevailing theories as to the nature of these disease processes. But both the older and the more recent systematic writers so treat of this ætiological question as to make the proof or disproof of it almost equally difficult. Thus, the action of cold in causing disease, is held to be mixed, so that although it is said to be either a predisposing cause or an exciting one, or both, these are dealt with together as one action, or at least not distinctly separated; and generally it is spoken of as capable of being supplanted or represented by some different agent.

Perhaps the greatest reliance is placed upon the statistical records of mortality and sickness, and upon this evidence the general statement is made

that "any considerable fall in the thermometer below the average standard, during the colder months in the year, is constantly followed by a corresponding rise in the death-rate, and an increase in still greater proportion in the amount and extent of sickness and suffering."

Statistical evidence is also urged to sustain the view that cold is the chief causative factor in the production of many particular named diseases.

(Several tables and charts were exhibited, showing the seasonal prevalence of some of the "chill diseases," but as it would be inconvenient to reproduce them, only the conclusions which are founded upon them are here given.)

There is a general consent and agreement among the authorities, that in all parts of the continent of Europe, in Great Britain and in Ireland, pneumonia, bronchitis, and pleurisy, are more fatal, and presumably more prevalent, in the colder quarters of the year, and are everywhere less so in the summer quarter. There are, it is true, some divergencies as to the extent of the immunity afforded by the warmer season, and as to the degree of fatality of the different cold quarters in different countries, as well as to the degree of correspondence with season shown by the different diseases named. But the constant, amid the variable, stands clearly out: that for the above named diseases the cold seasons are the most dangerous. This conclusion is further

strengthened by Longstaff's diagrams,* which show the fluctuations per cent. above and below the mean of the yearly death-rate from phthisis, bronchitis, and pneumonia in London for the years 1850-82, compared with a curve showing the number of cold days in the year. The correspondence for the last two named diseases is very evident, although not very close. It does not exist for phthisis. Longstaff's curves giving the weekly fluctuations per cent. of deaths from bronchitis and pneumonia in London for the years 1878-82 above and below the mean weekly mortality for the five years, also show that for both these diseases the mortality is greatest during the colder seasons and in the colder years. Bronchitis varying from the mean more than pneumonia, each corresponding evidently with cold to a considerable extent, but neither completely.

(Tables were also shown from which it appeared that the prevalence of acute rheumatism is greater in winter and spring than in summer and autumn.)

Attempts have also been made to trace in the geographical distribution of these diseases throughout the world a similar correspondence of their prevalence with cold. But a numerical expression of this is not to be had. In general

* See Transactions of the Epidemiological Society, 1882-3.

terms it is taught as to bronchitis and pneumonia, that they are universally but unequally distributed; and both are more frequent in high or temperate latitudes, or elevated and exposed situations, although neither are absent in the warmest. It is generally held that bronchitis more than pneumonia increases in the higher latitudes. Both are said to be relatively infrequent in some equable warm and dry climates. With respect to both, the distribution is rather capricious and irregular, and the information wanting in precision. The authorities on this matter usually do not consider cold apart from other things, but with "climatic and weather conditions," whatever that may mean.

The common experience of the profession and of the laity, however, appears to be much relied on as evidence in favour of this doctrine, and most of the writers of text-books appear to rest with such confidence upon this basis as to tender but little other evidence. Sometimes instances are recorded of prompt illness after the application of severe cold, but these are relatively very few, and not therefore of great value. Still it may be freely admitted that the general consent of competent observers in the profession is very strong evidence, and, indeed, it has so impressed me that I have only, with fear and trembling, ventured upon this criticism.

§ 3. THE DOCTRINE CRITICIZED.—Whatever

may be thought of the adequacy of this basis of proof for the superstructure of doctrine it aims at sustaining, it cannot be denied that the proofs ought to be as stringent for this as for any other ætiological teaching, if assent is to be given to it, and if it is to stand successfully the test of practical life as do true theories of causation. If the proofs are insufficient and doubt remains, it is better to say so. Let us not forget that, to the medical man, the value of his knowledge of ætiology is measured by the certainty with which he can prevent the disease, by avoiding the cause or cure, by removing it.

This doctrine rather invites criticism by the vastness of its claims upon our belief as well as from its importance.

Let me first note that it is somewhat in a state of decadence; and although it is still supported by a large majority of the foremost teachers of the day, in some form or other, and with certain modifications and reservations, yet there is a steadily increasing number of dissentients, and already some of the maladies which once were with perfect confidence imputed to cold, are now, by a considerable amount of general assent, put into another category. Cold being thus displaced somewhat from its importance as a cause of these diseases, and either neglected altogether or considered only as a less important factor. The diseases which have, in the progress of science

and the experimental methods been separated from the category of chill diseases, are those which are best defined clinically and pathologically; their true ætiology and pathogeny have been proved by more exact methods, the conclusions have been submitted to stringent criticism, and have survived it.

So must the residue of these diseases be defined, their ætiology and pathogenesis be shown by equally stringent methods, submit to similar criticism and survive; or the admission must be avowed that the causes are not known. So that encouragement may be given for further research, and not an inducement to remain satisfied with an unproven hypothesis.

Tubercular phthisis is the best example of a disease well defined, and established upon a fairly sure pathogenesis, formerly imputed to cold, and now removed from the category upon evidence widely accepted as sufficient.

Croupous pneumonia, whatever may be said about Friedländer's pneumococcus, is also, in the opinion of many very competent authorities, justly removed from the group of chill diseases, and to quote the opinion of Jürgensen, "The assumption of a specific disease exciter is necessary." . . . "External injuries, including in that expression taking cold, can scarcely possibly be held to be the exciting cause, considering how rarely it is met with as an antecedent." He considers that it

belongs to the group of infective diseases: and I would add that, as it is admitted that at least some forms of croupous pneumonia are infective in some way, and as these forms are not always clinically distinguishable from the sporadic cases, it seems certain that these forms should be separated from the chill diseases, and thus an argument from analogy is founded against retaining the remainder in that category.

To these two I would add some of the varieties of coryza, sore-throat, bronchitis, pleurisy, rheumatism, Bright's disease, and some others. That it is to say such as are caused by other known agents, such as morbid poisons, dusts, and drugs.

There remain for consideration the non-specific forms of the same maladies,—in other words, those forms of which the definite cause is unknown. I must observe, however, that many writers consider cold as an ætiological factor of importance, in some unknown way, even in the specific forms of these diseases: nor do I wish to deny that it is a factor, although I cannot attach the importance to it which is commonly given. In considering this remnant, which is a sufficiently large group of serious diseases, I must ask you to pardon me if I, in order to clear up the position, enter upon some general comments, which might at first seem to be a needless digression, yet appear to me needful.

The problem to be solved appears to me to

have been stated with too little precision to permit of its being worked out by the more exact methods used for the solution of other ætiological questions. Using the terms cause and effect so as to make our conclusions certain, we must say with Sir Thomas Watson* that "one event is held to be the cause of another event which follows it, when, the first being absent, the second never occurs; and the first being present, the second never fails to occur, unless some other event intervene to prevent it." But were this rigid method adopted there would be no more discussion, for we should say at once these diseases are not caused by cold. In medicine such strict methods are not in use very much, and are seldom possible. Yet in proportion as we use less stringent tests, we get uncertain or indefinite results. The same excellent teacher has given us another formula, more suited to our difficulties if somewhat less exact, thus, "We perceive that certain external circumstances (*quæ nos circumstant*) often precede such and such diseases, and that the diseases seldom happen when the same circumstances were not previously observable; and we begin to regard those circumstances as exciting causes of those diseases. We find that the diseases are much more frequent among persons known to have been exposed to the agency of the suspected causes than among

* Lectures on the Principles and Practice of Physic.

persons who are not known to have been so exposed. The evidence at first is presumptive only. But the more uniform their conjunction and the more rare their disjunction the more confidently do we assign to the two consecutive events the relation of cause and effect. By this kind of observation a number of exciting causes of disease have been clearly established to be such."

This rule, doubtless, may yield useful but not certain results, and I fear it is seldom employed with due rigour. The one in most common use is simpler and less exact, and is expressed somewhat thus. If an illness is preceded by an apparent outward change like exposure to cold, and if that outward change be one which is commonly held to be competent, such change is called the exciting cause; but as not all persons so exposed, but only some of them, suffer from the said illness, those who do suffer are said to be predisposed in some way, and such predisposition is attributed to the previous operation of causes, which are hence called predisposing. This method is easy, and therefore tempting, but it is not the less beset with pitfalls, into which I fear we all fall now and then.

Yet have we not been without good examples; for there are maladies of the simpler kind, to the investigation of which it has been possible to apply the simple strict method, in which the causes have been proved by invariable antecedence, by equality

of effect, and by promptitude of action. For examples, turn to the simpler skin diseases, surgical affections, and the effects of poisons and drugs, as well as some general diseases, of which syphilis, tubercle, and small-pox are illustrations. One great reason why it has been possible to reach such certain results, has been that the maladies have been capable of definition, and therefore the question to be answered could be put clearly and precisely.

Quite otherwise is it with the diseases attributed to cold. These form a series of groups not as yet separated into well defined clinical units. So that the ætiological problem cannot be put in a precise form, the tests be properly applied, or a definite response be expected. In illustration of this I point to the various catarrhal inflammations of the nose, throat, and bronchia, as examples, in which clinically we often cannot distinguish the forms even when they are known to have diverse causes. Were it possible to separate these groups into disease units, that is, were the inflammations due to the different causes distinguishable, so that each could be ætiologically definable, it would become possible to state the question with sufficient precision, to apply accurate methods, and to obtain trustworthy results. This is not so now. We are taught instead that the same cause may produce many, and very different diseases, and *vice versâ*. Which is as much as to say that a cause is a

variable and inconstant antecedent, and neither measures the intensity nor modifies the quality of the effect.

Now no one doubts that events are often determined by any one of many disturbing agencies or causes, or that the same cause may produce, under differing conditions, many different effects. We, least of all, can doubt this, when in practice illustrations abound. See the person poisoned by strychnia, thrown into convulsions by a hot or cold contact, a sound, or a beam of light, and other such cases where a state of unstable equilibrium exists. I need only say here that these variable excitants of convulsion are not the true causes, and are but the occasional, *i.e.*, mere accidental causes: the true causes, the knowledge of which would be helpful to us to enable us to diagnose, prognose, prevent, or treat the malady, being in all such cases those which brought about, not the paroxysm, but the state of unstable equilibrium. With respect therefore to all those causes which may be replaced by other disturbing agencies in the production of the event they are relatively unimportant, and if known to us, would not help us much; seeing that in such cases, were the sufferer to succeed in staving off one of the accidental causes of the attack, his susceptible condition would leave him a prey to the next disturbing agent of whatever kind, and the result would be the same. Further, it is to be remem-

bered, that but few of the diseases I am dealing with can lay claim to be considered as cases of unstable equilibrium, and most of those that can so claim, as asthma, &c., are separable from cold-caused diseases.

Similarly, we all know that the general disease processes common to different diseases, as inflammation and fever, are produced by many different causes. But it by no means follows that particular diseases are so. I will, later on, attempt to show that a special ætiological factor is characteristic of each named disease and with the name we simultaneously conceive of the pathogenesis of such disease.

Therefore, the sort of evidences we have had to consider, and the methods used for ascertaining the causes of these diseases, cannot escape criticism by an appeal to cases of unstable equilibrium, or to the general disease processes.

Whatever value the statistical evidence for this doctrine may have, it is admitted to be imperfect. Dr. Longstaff* with true scientific caution says, "It must not for one moment be supposed that it is alleged that heat, cold or drought are the causes of these diseases," and after an elaborate investigation he is content to conclude that there is a correspondence between the death-rates and certain meteorological conditions, which indicates that such conditions are favouring, or predisposing conditions.

* l. c.

Thus he says, "The mortality from scarlet fever will be greater in a dry than in a wet season. More will die of bronchitis in a cold winter than in a mild one, more of diarrhœa in a hot summer than in a chilly one." I should put it: the statistical evidences tell us not the causes, but some conditions of their action.

Should anyone be disposed to hold that the very striking correspondenee shown between the curves of bronchitis and pneumonia, and those of eold, indicates a true causal relation, I would ask him to refer to the paper by Longstaff above spoken of and to his diagram showing the prevalence of continued fevers in London.* These establish a marked increase of the mortality from, and prevalence of typhus in the colder months of the year; and a certain prevalence of enteric fever is observable for autumn extending well into the winter. Indeed there would seem to be about as much reason to impute typhus to eold as there is for bronchitis and pneumonia. On similar grounds one should, to be consistent, refer diarrhœa to heat, and scarlet fever to dryness. But it is well known that most diseases show seasonal fluctuations, and some, like small-pox, measles, and whooping-cough do so in a marked degree; yet these are not imputed to variations of temperature.

We must also remember that statistical data

* Trans. Epidem. Soc., 1884-5.

are imperfect, and errors arise therefrom requiring much caution in dealing with the results. Thus Longstaff* points out that the death-rate from phthisis fell in twenty years 20 per cent., although in the same period from all respiratory diseases it had risen 5 per cent.; and he explains this apparent decline by variations of diagnosis and nomenclature. The great apparent rise of the death-rate from bronchitis, in twenty years, amounting to an increase of 81 per cent., taken together with an apparent diminution of the death-rate from pneumonia of 20 per cent., are also instances of great variations in diagnosis and nomenclature. So also there is much reason to think that the 4376 deaths imputed to simple continued fever in sixteen years in London are in some way wrongly so imputed, as that disease is rarely or never fatal when treated in hospitals.

The topographical distribution of these diseases furnishes further grounds for thinking that other factors than cold dominate in their production. I refer you to Longstaff's tables† showing the death-rates for bronchitis and pneumonia per 1,000,000 living in the registration counties of England and Wales. Whether we consider these diseases together, or separately, the tables show that the mortality is higher in the Metropolitan districts than in the extra-Metropolitan districts immediately adjoining; although it would be hazardous to

* l. c., 1882-3.

† l. c.

assert that the densely populated urban districts are the colder. The other registration districts do not lend themselves so well to this sort of comparison; perhaps because in many of them dense and sparsely populated areas are contained in the same unit. However, a consideration of these tables strongly suggests the idea that some of the conditions of life met with in the industrial districts favour the prevalence of these diseases, more than cold and other such climatic influences do. Thus Lancashire is the most deadly district for both of these diseases, adjoining Westmoreland is one of the healthiest, and almost as much may be said for Northumberland and Norfolk. In short, whether in the north or in the south, east or west, at high or low elevations, the general rule seems to be that rural districts are less exposed to these risks than industrial ones. At my request Dr. Whitelegge, to whom I owe most sincere thanks for his able and generous help, tried to prepare curves showing the relation between density of population and the prevalence of these diseases. I am sorry that available data have not so far been found so as to permit this to be done satisfactorily. From Dr. Whitelegge's figures, however, it is obvious enough that mere density of population does not, any more than cold, truly measure the prevalence of either of these diseases.

(A table was exhibited, copied from Hirsch,

giving the annual cases of rheumatism among British troops at home and abroad. In this it was shown that the station least liable to rheumatism is Nova Scotia, that Canada is less liable than Great Britain and Ireland, which are less prone than the Mediterranean, the Indian, and the Australian stations. So that the common theory finds but little support from these returns.)

The statistics presented in evidence of the validity of this doctrine count only the instances in which the disease is present, and do not adequately deal with those instances in which the cold has been operative without the occurrence of the malady. The ratio of the attacks to the population is given, but this does not suffice. So that on this ground also the value of this evidence must be challenged. It does not suffice to compare the number of persons living under varied conditions with the number of those who suffer a given disease, when it is accepted that not one alone, but many of those conditions may be singly competent to act as the cause. Nor does it sufficiently remove the difficulty to classify the people according to the sets of conditions under which they chiefly live. Only when the conditions suspected, and the maladies under consideration can be defined and isolated, are positive results to be attained. This at present cannot be expected from public statistics, but something like it may be hoped for from the

experimental method. Yet the experience of life tells, of the millions who toil, as of the hundreds who play, that for one or other reason exposure to cold takes place thousands of times in a lifetime,—even of those who at length suffer from one or other “chill disease”—for once that such an event occurs: and the majority of people are so exposed during a whole lifetime and never once suffer, at least from the graver forms of these diseases. However, if we could find out the true ratio between exposures, say to cold or heat or varied climatic influences, and the occurrence of certain diseases, we might at least calculate the risks: but it would not follow that we could protect ourselves by avoiding such exposures, if the theory be true that other causes are competent to produce the disease.

Also we should count those contrary instances seen in various therapeutic methods. Witness the remarkable applications of cold locally, or generally, in the various Hydropathic institutions; the free use of cold in many surgical affections, and especially I ask you to consider the significance of the treatment by cold in the shape of ice, of very cold water in tubes, etc., and of baths in some of these “chill diseases” without injury and often with much advantage. See also the treatment of phthisis at high levels in very cold air. It will be seen that we employ therapeutically; both as cold air breathed into the parts affected, and as cold

water applied to the surface locally or over the whole skin, either for short periods, or for long, either gradually or suddenly, and after previous heat and perspiration; just the very agent which is credited with being the chief, although not the only cause of these affections.

But, as I have already said, perhaps the strongest evidence for the current doctrines as to cold is the general acceptance they have received in the profession, and the concurrence of opinion shown by systematic writers. This, I confess, has been for me a very great difficulty, and has almost persuaded me to avoid so unequal a contest. However, as this question has in one or other shape much occupied my attention for more than a quarter of a century, and I have found myself as time went on more and more convinced of the weakness of this teaching, and of the injurious effects of its adoption, I have at length summoned up courage to make this attempt in the presence of a friendly audience to show that it does not rest upon a sure foundation. There will, I think, be no refusal to admit that the antiquity of the doctrine, together with its almost universal adoption, has created a widespread readiness to accept without criticism statements which seem to accord with it; a feeling which one party would call a presumption in its favour, and the other a prepossession for it; which in any case does constitute a danger needing to be guarded against in collecting facts

and arranging them as evidence. Even medical men, if I may judge by my own state of mind, are not free from the influence of widely spread notions of pathogeny, and mostly I think we feel this in early life.

Neither are we altogether free from risks of error due to a certain indolence of mind, which tempts us to accept statements, which are but inferences, as if they were facts, without submitting them to a criticism which is always troublesome, and often distasteful, both to the patient, and to the doctor. Older men too have to admit that there is frequently a temptation to arrive at conclusions which harmonize with those of ~~our~~ clients, and therefore are more agreeable to them. The importance of these considerations appears, when we consider that most of the facts relied upon as evidence are but statements made by patients, who for the most part quite unconsciously mingle opinion with fact, and give their confident inferences as if they were observed facts.

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It has been my custom for nearly thirty years to endeavour to obtain definite statements from my clients as to a particular instance of exposure to cold, whenever the malady has been imputed to that agent: with the result that only in a very small minority of cases has such definite instance been remembered. For the most part, patients conclude that they must have "taken cold" because they suffer particular symptoms

commonly imputed to cold. Often an instance of exposure to cold is mentioned, not at some appropriate time before the disease began, but after it has already made some progress. This is especially frequent in those cases in which sensations of chill, or rigors, attend the early stages of the disease. Often an instance is stated of such trivial exposure as could scarcely be accepted as adequate, even by the most credulous. The general outcome of my experience has been to show that if we scrutinize a patient's evidence, and take only statements of fact and not statements of opinion, which are the result of the notions of pathogeny adopted by the witness, the number of instances in which the sufferer remembers a special instance of exposure to cold at an appropriate time before the disease began, is an extremely small proportion of those who suffer from a malady imputed to cold. How infinitely small then must be the ratio of attacks to the total exposures if we include the instances of moderate exposure as the doctrine requires us to do!

So that what Jürgensen says of pneumonia I would extend to many of the cases of bronchial catarrh, of acute rheumatism, and of pleurisy, viz. that as only a minute fraction of those persons who are exposed suffer, and those who do suffer do so only in a minute fraction of the instances of their exposure, and as many suffer without any such exposure, there is no sufficient ground for imputing

such diseases to cold as their cause. But you will naturally, and I admit properly, say, of what value is your personal experience against the general consent of the Profession? I do not presume to defend my position, and can but say that I am convinced, and that conviction is not a voluntary matter.

There are also individual cases met with in the experience of medical men, and doubtless all of you have had such, in which after marked and severe exposure to cold serious disease has appeared so immediately as to raise a presumption of a causal relation between the two events. In respect of these cases, it is to be noted that mere promptitude of sequence alone, is of little value: although it is of importance when it coincides with some degree of constancy, and especially when there is a certain equivalency between the two, *i.e.* when the antecedent measures the consequent. In these cases the mental condition of the observer is of extreme importance and is very apt to mislead if he be not entirely free from prepossessions. The recorded cases I have met with are but few, and they are, I presume, not very frequent in practice.

I attach great importance to the argument against the validity of this doctrine, that it is difficult to reconcile with our ideas of pathogenesis derived from the study of the simpler and better known affections, *e.g.* the surgical maladies, those of

the skin, the eye, and those experimentally produced. This doctrine we are criticizing says, catarrhal inflammations of the respiratory, the alimentary, and the urinary organs, with and without attending febrile action, fibrinous inflammations of the lung and the pleura, rheumatic inflammation of the organs of locomotion and circulation, acute and chronic, locally infective, spreading sometimes even from person to person, endemic and epidemic, are mainly caused by cold, although sometimes by other agencies. Certainly nothing like this could be said of the simpler and better known diseases. Further, we are asked to admit that cold may act directly upon the part affected, as when a coryza or a laryngitis is imputed to the cold air breathed: locally, but in some less direct way when a sore throat, a bronchitis, a pleurisy or a pneumonia is imputed to the action of a cold draught upon the throat or chest: indirectly, and from a distance in some way, when any of the "chill diseases" of viscera are imputed to some dampness or cooling of any indeterminate distant part of the surface.

Compare such a teaching with what we learn from the observation of surgical cases, skin diseases, and certain fevers. From these we learn that the effect follows the action of the cause—conditions being constant—with very considerable certainty, that it is much measured by the cause, and that the two are so correlated that any variation of the causal agent, or of the subject of

its action, is associated with a corresponding variation in the disease resulting. Thus, for these affections there is such a perceived relation between the malady and its cause, that the latter may often be inferred from the observed characters of the former. As when from the characters of an iritis, or a skin disease, we conclude that the person has been affected by the syphilitic virus, and so on. Who can say the same of the "chill diseases"? Our notions of ætiology and pathogenesis are thus intimately associated, and indeed inseparable: but I see no way of reconciling them with the doctrine that cold produces so many and such very different diseases.

The doctrine of the causation of disease by cold is also open to strong objection upon the ground that it conflicts with our notions of the pathogeny of inflammations. And seeing that nearly all the diseases imputed to cold are inflammations of some sort, this consideration is appropriate. We, perhaps, best conceive of inflammation as the reaction of a living tissue to an injurious irritation; and the rule is, I think, constant, that when the irritant is a simple mechanical or physical one, the resulting inflammation is in direct proportion to the irritation, and limited nearly in extent to the area irritated, as well as in time to a short period beyond that of the action of the irritant. It will be seen that this view assumes the presence of some irritant body or agent at or near the seat

of the inflammation. In cases, however, in which other added causes of irritation intervene, the problem is not so simple, and the characters of the inflammation then vary with the properties of the additional irritants. These views are demonstrated by the triumphs of modern surgery. Therefore, seeing that cold is one of the simpler disturbing physical agents, one would expect that the maladies produced by it would follow the rule, that is, be limited in area to the region cooled, be brief in duration, except when the cold was long applied, soon subside after its removal, and be measured as to severity by the intensity of the applied cold. This is so, in fact, as regards all the inflammatory effects of cold about which no one doubts, as in frost-bites and chilblains.* But this is not so with the visceral inflammations imputed to cold. In these cases the effects appear at a distance from the part cooled, are prolonged even when the exposure has been brief, are disproportionate in severity to the intensity of the imputed cause, are indeterminate in site with relation to the seat of the applied cold, and not infrequently spread, not only in the organ or tissue affected, but from person to person as do infective diseases.

* There is much reason to think similarly of the fever imputed to cold. As yet we have not as much knowledge of the febrile as of the inflammatory process, but I think it may be said that the febrile states which can be produced by chemical substances, and indeed all those of which the causes do not multiply in the system, are of brief duration.

So that these characters resemble very little those of inflammations certainly due to cold, but are very similar to those of the maladies due to morbid poisons of some kind. Here let me repeat that no one doubts the power of cold, as one of many disturbing agents, to derange a delicate mechanism, and thus set up the first step of a series of actions spreading beyond the part first affected, enduring for a longer time, and disproportionate in gravity to the original disturbing agent: but in such cases the malady is not in any important degree dependent upon such accidental cause; and cannot be prevented by avoiding that particular agent, seeing that any one of many other disturbers not only may, but almost surely will happen, and bring about the disease in such a state of hypersensitiveness as is here under consideration. There is some reason to think that many of these maladies are traceable to the co-operation of more than one causative factor, and that cold or other climatic influence may be one, some pathogenic organism being, however, the more essential one, as is presumably the case in certain surgical maladies in which the mechanical lesion makes possible the action of some particulate poison.

I am tempted here to suggest an hypothesis as to these internal inflammations which satisfies my mind better than the current one. Grant then, that all diseases like other effects are the products

of their factors, and vary with them, and it follows, that every inflammation varies in correspondence with changes either of the irritant which produces it, or of the reacting tissue. I would therefore say, whether we are able to distinguish it clinically or not, that there is a real and specific distinction between the bronchitis caused by, say cold, or by the pollen of certain grasses, or by an overdose of Potassium Iodide, or the morbid virus of measles; and similarly between the pneumonias which are caused by diverse irritants. This would apply also to fevers from different causes. It would result in the formation of probably nearly as many species of inflammation of the internal organs as we now accept for the skin. Indeed, I think the reason why we have formed so many species of dermatitis is less the greater exposure of the skin to irritants than its exposure to our investigation.

There is also an argument opposed to the prevailing doctrines on cold as a cause of disease, derivable from our notions of evolution, which weighs strongly with me. I know not how far you may be disposed to attach importance to it, but if you will bear with me I will very briefly state it. Thus, inflammation may be looked upon as a faculty of all organized beings developed in the struggle for existence against external injurious influences: represented perhaps in the primitive cytod by its first formation of a limiting surface, or cell membrane, and shown next in the

nutritive changes in the cell wall by which it heals up such thin places as occur during the expansion due to growth, or to the more accidental injuries to which it is liable, and in the midst of which it lives. In higher and more complex plants and animals this faculty of inflammation is represented by cell multiplication with modification, and special provision for the supply of nutrient fluids to the parts irritated; and it results in a more or less perfect repair of the damage done. Among the external injurious influences to which all organized beings are exposed, those which are most universal and constant, and those to which the organisms have been longest exposed, have of necessity been most perfectly guarded against, by the development of these powers of reaction, without which they would not have been able to persist as species. Thus, among plants and animals within certain limits, there is a complete adjustment for variations of temperature, of dryness, and of moisture; so that within those limits such variations are not only harmless, but advantageous to the individual and to the species. Even beyond those limits for a moderate amount, plus and minus, the protective reactions are sufficient to secure the organism from very serious consequences so far as regards mechanical and physical injuries. But for some of the chemically, and especially for the organic and organized injurious agents, which presumably have been less con-

stantly, and for a shorter period of time, operative, a less perfect protective faculty has been developed; and the reactions they excite are often irregular and even destructive, as may be seen in the ravages caused by parasitic organisms among our cultivated plants and domesticated animals, as well as in ourselves. Granted that here also some wonderful adjustments have been developed such as are shown in symbiosis and commensalism; but these exceptions do not weaken the general truth of the statement, that parasitism of some kind is the most frequent if not the sole condition of continuous or infective disease processes in plants, animals, and man. Conformably with these views, it is improbable that variations of temperature would produce such effects as are imputed to cold, and it is more reasonable to expect the reaction of living tissues to cold and heat to follow the law which obtains for mechanical impacts of which we see such convincing proofs in surgical practice.

§ 4. ITS INJURIOUS RESULTS.—Whatever may be thought of the value of these criticisms, there is no doubt that the doctrine accepted by the laity as to the relations of cold and disease has determined for them a conduct of life which does much harm. I should say that the extreme precautions most people take against cold expose them to greater risks from impure air, want of cleanliness, and of exercise in the open air, than any they can

avoid by those precautions. For it is manifest that a habit of living much indoors, with imperfect ventilation, warm rooms, and excessive clothing—not sufficiently reduced even in summer—must—according to the doctrine accepted—make people more sensitive to “cold or the other climatic influences” to which sometimes all must be exposed. So that double risks are run, viz.: those so much feared of cold and those perhaps more serious, really due to impure air, an uncleaned skin, and an inactive body.

In short, the current doctrines have resulted in a habit of life, by which delicacy is cultivated until even a brief exposure to the temperate winds of these islands does produce discomforts, and these, exaggerated by fear and a confused popular pathogenesis, are mixed up with graver evils of other origin. The diseases really induced even in these sensitives by cold pure air are trivial by comparison and soon pass away. The prevalence of this over-care to avoid a danger, which is exaggerated; by blindly incurring other risks, has produced a wide-spread reduction of that which in its entirety is called robust health; and by it the average vigour of the people, especially in great towns, is reduced. Some of the domestic habits of the poor are influenced by these idle fears in a way which makes their position yet harder. Witness the habit of closing the throat of the chimney in the bed-room, always

a small one, the closed windows, the crowded furniture, and the air so loaded with impurities as to be unfit properly to sustain a healthy respiratory process.

The fear of draughts also creates difficulties for architects and managers of public buildings. The impossible task is required of them, to keep a crowded assembly supplied with air, fresh, and pure enough for comfort and health, cool and yet warm enough, but not moving at a velocity which enables anyone to feel it. The requisite quantity of air per head per hour cannot possibly be supplied under such limitations. Yet were this superstitious dread of "a draught" once overcome, the due supply could be given so as to provide for health and comfort. Were I able to regulate these matters in public assemblies, there should be a fixed minimum of air per head per hour supplied at a fixed temperature, and in other respects so as to be conducive to health, and not uncomfortable to an unprejudiced person of average health. Such arrangements should not be varied at the desire of the more timid and ignorant of the audience, but each person would have the right to demand that the rules should be duly observed.

The domination of these doctrines among the laity adds much to the difficulties of the medical attendant who knows the great need in feverish cases and inflammatory affections, especially of the

organs of respiration, of an adequate supply of pure fresh air at a moderate temperature. Not less does it stand in the way of many beneficial cleansing and cooling processes now so much recommended and used by the best of our teachers. So that we should gain, as well as our clients, were they to hold sounder views on this question.

Questionable comfort may perhaps be derived from the somewhat inconsequent way in which many patients who consider heat the natural cure for the cold-produced diseases, follow any converse practice like hydropathy or the air-cure in these same maladies if only the fashion prevails.

Some medical men, too, who impute croupous pneumonia and acute rheumatism to cold, treat them freely with cold applications long and often applied. In this way the uniformity of the tradition is broken, even if it be difficult to see the logical consistency of the practice and the theory.

§ 5. SUMMARY AND CONCLUSION.—In the foregoing remarks I have endeavoured fairly to state the current doctrines as to the influence of cold as a cause of disease, the grounds upon which they are defended, the want of definiteness and consistency they exhibit, both as regards the diseases imputed to cold, and as regards the mode in which it is said to act. I have tried to show that the evidences relied on do not suffice to prove the doctrines. That they show only a greater prevalence of those diseases in cold and variable

seasons, similar in kind, if less in degree than the prevalence of certain other diseases which no one thinks of imputing to cold, and similar also to the prevalence of certain other diseases in hot seasons, yet not due to heat as the cause; and that we can only infer that heat and cold, are in these diseases favouring conditions for the action of the causes. That even for estimating risks the doctrine is not safe until it reckons the contrary instances, and that were this done the risks would be shown to be fewer and less serious. That the graver maladies of internal organs which are imputed to cold, have few or no resemblances to the affections known with certainty to be due to it, or to the affections caused by allied agents; but do much more resemble some acute inflammatory and febrile visceral affections, known to be caused by certain morbid poisons; so that upon analogical grounds the probability is great that some allied causes are the efficient ones in these diseases, or at least in a large proportion of them; and that such of them as may be due to the action of cold, are brief in duration, limited in area, near to the parts acted upon, not infective, and not chronic unless the cold be repeatedly or continuously applied. That it is a more defensible and a more definite doctrine to consider the inflammations of internal viscera and tissues to be numerous in species, and like the inflammations of skin variable as the causes. That the true causes of many of these

diseases have yet to be determined. That the domination of these views among the laity is an injury to national vigour and a hindrance to the advance of a rational Therapeutics.

I venture to indulge the hope that to the minds of some now present I may have given reasons which will at least induce them to reconsider these doctrines; and should any of them be led to throw off the fetters they have imposed, I am confident they will, in their own lives, derive advantages of personal health, and in their practice confer benefits upon their clients. Should it be, however, that no one present is induced to call in question the commonly accepted doctrines on this question, I shall feel, not that the doctrines I have attacked rest upon satisfactory grounds, but that the attempt that I have made to exhibit their weakness has failed on account of my inability clearly to set before you the reasons for refusing your assent.

Were, however, the Profession and the public once freed from the restraints due to the prevalent fear of cold, I should anticipate among the benefits of such emancipation, a more active search for the true causes of the various diseases now wrongly imputed to cold, a more rigorous analysis of the groups of symptoms met with in the various forms of those maladies, such as might enable us, on clinical grounds, to recognize true species of rheumatism, pneumonia, and bronchial inflammations, &c., &c., a greater freedom therapeutically

in using fresh pure air, as well as cooling and cleansing appliances, and generally a willingness to encourage our convalescents and healthy clients to expose and exercise themselves more freely in the open air, as well as to maintain within the house an air supply as pure and fresh as that without.

Thus I should expect that the people freed from a host of idle fears would be led by the teaching and example of the profession to act upon more rational doctrines; and would find out ere long that for a healthy existence, for the attainment and maintenance of a constitution capable of successfully resisting unavoidable exposures, it is both safe and pleasant to live out of doors as much as time permits, within doors to keep the air as much as possible like the outer air in point of purity, and so to use clothing and artificial heat that they may suffice for comfort and no more. Thus, we should cease to cultivate delicacy in our midst, without incurring risks worthy of being considered, against the double dangers, which people who coddle, are exposed to.

At the best, in life, we have but a choice of risks, and no course of action is entirely free from them; but the mode of living I have tried to depict as desirable, is that which exposes to fewest risks and to those of least gravity; while it possesses the unusual recommendation of being pleasant as well as prudent.